

Note on Mr. Christie's Paper in the *Monthly Notices* for May 1881.  
By E. J. Stone, Esq.

The remarks which Mr. Christie has inserted in the *Monthly Notices* for May 1881 are unfortunately of such a nature that they cannot close a controversy.

I certainly have not put words into Mr. Christie's mouth and charged him upon the strength of these words with fallacious reasoning. The fallacy which, in my opinion, invalidates Mr. Christie's work on these questions of refraction can be clearly stated, and it depends upon no misrepresentations of mine.

I have proved, by a re-calculation, that the residual errors given in my paper of 1867, December, are substantially correct: and that the Greenwich circumpolar observations 1857 to 1865, to the lowest altitudes observed, are well represented in mean results by refractions computed from the formula Bessel's Refractions ( $1-0.005$ ). I have also shown that the Greenwich circumpolar observations which were reduced with these refractions, 1868-1876, are well represented. I have shown that Bessel's Refractions *unaltered* do not represent these Greenwich observations. These statements are mere matters of fact; the residual errors which prove them have been given over and over again in the course of the present discussion. The agreement between the Greenwich circumpolar observations and the diminished refractions may, of course, be solely due to systematic errors in the Greenwich results; but this is improbable in itself; and, if it were true, it would not prove that the change in the refractions adopted at Greenwich in 1868 was due to an error on my part, or was made upon *slight* evidence. The evidence afforded in favour of the diminished refractions by these Greenwich circumpolar observations is, as I have elsewhere said, irresistible, unless some other alternative hypothesis can be brought forward which shall equally represent the facts, and which has *equal claims to be accepted as a possible solution of the problem of Astronomical Refraction*.

In 1868 we had no such alternative hypothesis. The broken, discontinuous, curve system with its three distinct tables of refraction, then in use at Greenwich, was considered only an unsatisfactory make-shift. The breaks of continuity, introduced into the adopted refractions, were fully recognised as impossible in nature. They had no other *raison d'être* than the facts, proved by Main, that if you would adopt Bessel's Refractions unaltered for the smaller zenith distances, and determine your colatitude under the same restrictions, then the error of the colatitude, thus determined, and the errors of Bessel's Refractions did not become sufficiently separated to be put clearly and distinctly in evidence until you reached a zenith distance of about  $82^\circ$ . But the error of Bessel's Refractions, in mean results, appeared to be

about  $0''\cdot9$  at  $82^\circ$  zenith distance, whilst at  $85^\circ$  zenith distance the error had increased to about  $2''\cdot9$ . It was considered necessary, for practical applications, to represent the observations at low altitudes, and this was effected by the very simple expedient of throwing away the mean errors which Mr. Main had shown to exist in Bessel's tables. The system adopted, to effect this, was the use of three independent refraction tables—Bessel's Refractions to  $82^\circ$ ; Bessel's Refractions  $\times 0\cdot9977$  from  $82^\circ$  to  $85^\circ$ ; and Bessel's Refractions  $\times 0\cdot9951$  below  $85^\circ$  zenith distance. The arbitrary breaks thus introduced show at once that such a system cannot exist in nature. No proof of the legitimacy of the plan adopted has been attempted. But the large errors can, of course, no longer appear as "residual errors" in a discussion of observations reduced with refractions from which they have been thus arbitrarily removed; the smallness of such "residual errors" only proves that the observations under discussion are in substantial agreement with those from which Mr. Main originally determined the adopted corrections. It is the errors of refractions computed from the discontinuous, broken curve, system, abandoned in 1868, which Mr. Christie has compared in his curves and residuals with the errors which result from the use of refractions computed from the formula Bessel's Refractions  $\times 0\cdot995$ . The latter formula is continuous: it requires no greater number of disposable constants than are contained in Bessel's theory to represent in mean results all the Greenwich circumpolar observations 1857–1876 from the zenith to the lowest star observed. The refractions used by Mr. Christie are discontinuous. They are not computed from a single table: and, to secure an equal agreement between the computed refractions and the Greenwich observations, it requires three disposable constants instead of the one required by the refractions introduced in 1868. It was simply on these grounds that the diminished refractions were adopted in 1868 by Sir G. B. Airy.

It is the comparisons which Mr. Christie has instituted as *an alternative hypothesis*, to weaken the evidence in favour of the diminished refractions, between residual errors resulting from the use of refractions computed from this complex, arbitrary system with those arising from the use of Bessel's Refractions  $\times 0\cdot995$ , which constitutes the fallacy which, in my opinion, vitiates Mr. Christie's reasoning and statements on these questions of refraction. Mr. Christie may maintain that there is no fallacy in the institution of these comparisons. He is, of course, fully entitled to the benefit of his own judgment upon the point. I, however, consider it a serious fallacy—a fallacy which entirely destroys the force of his remarks on the slight evidence which these Greenwich observations afford in favour of the diminished refractions. The mere fact that it requires such a complex system with two additional constants to equally represent the observations is a proof that the evidence afforded by these Greenwich observations in favour of the diminished

refractions is indeed very strong. At all events, the fallacy to which I have called attention, such as it is, is not of my making. It runs through all Mr. Christie's work on the subject of refractions. It is not due to words which I have put into Mr. Christie's mouth. I should be utterly ashamed of myself if such a charge could fairly be brought against me.

The notice, "The Nadir Point Observation is to be taken at  $179^{\circ} 40' + 0''$ ," to which Mr. Christie calls attention on page 344, meant exactly what it says—viz. that the readings were to be taken on the first revolution, or from  $0''0$  to  $1''0$ . This is shown by the general practice to which it led. The practice was adopted, as I have before stated, to keep the correction for runs small for the Nadir Point Observation. If errors such as those given in the Introduction to the Greenwich Catalogue exist in the readings of the screws near  $0''0$ , the supposed errors of the screws at other readings will change from the mere effect of these errors on the runs by such quantities as the following:—

*Supposed Errors of Screws.*

Runs started from	$0''5$	$1''$	$2''$	$3''$	$4''$	$5''$
$9''9$	$-0''1$	$-0''2$	$-0''5$	$-0''7$	$-0''9$	$-1''2$
$0''1$	$-0''1$	$-0''1$	$-0''2$	$-0''3$	$-0''4$	$-0''5$

I think that the magnitude of these differences will show that there may be some considerable advantages in keeping the run-correction small for an observation like the Nadir, which is common to all the observations of a night. If Mr. Christie cannot see the advantage of separating the effects of two distinct sources of error, I regret it, but I cannot help it.

Mr. Christie's statement that the constant error of  $-0''8$  in the Nadir observations was due to error of the screws about  $0''0$ , which had suffered extra wear since 1868 from a preponderance of some 4,000 observations taken at this part, was, I thought, sufficiently disposed of when it was pointed out that instead of 4,000 additional observations there were only 108 such observations made with readings at and below  $0''2$ , whilst there were 72 such determinations made at readings greater than  $1''0$  during a period of nearly two years—1868, February 1, to 1869, December 31. This would give less than 500 additional observations, instead of 4,000, during the period of 1868 to 1876. Mr. Christie, however, thinks that these facts do not concern him. The corrections which have been applied to the Greenwich Observations 1874–1875 for these supposed errors of screws are given in the following table, which is extracted from the Introduction to the Greenwich Catalogue, 1878:—

9.7	-1.38	1.4	+0.08	3.1	+0.38
9.8	-1.13	1.5	+0.08	3.2	+0.22
9.9	-1.17	1.6	+0.18	3.3	-0.06
0.0	-0.89	1.7	+0.12	3.4	+0.05
0.1	-0.51	1.8	+0.05	3.5	+0.31
0.2	-0.41	1.9	+0.22	3.6	+0.33
0.3	-0.42	2.0	+0.47	3.7	+0.27
0.4	-0.09	2.1	+0.55	3.8	+0.22
0.5	+0.28	2.2	+0.66	3.9	+0.21
0.6	+0.34	2.3	+0.76	4.0	+0.19
0.7	+0.28	2.4	+0.52	4.1	-0.05
0.8	+0.19	2.5	+0.39	4.2	-0.19
0.9	+0.22	2.6	+0.52	4.3	-0.03
1.0	+0.30	2.7	+0.42	4.4	-0.17
1.1	+0.23	2.8	+0.15	4.5	-0.39
1.2	+0.23	2.9	+0.15	4.6	-0.51
1.3	+0.23	3.0	+0.32	4.7	-0.74

I point out the extreme rapidity with which the error changes near 0.0, that on and after 0.5 the changes of the corrections are, comparatively speaking, small. It appears, therefore, that if these errors are due to serious wear of the screws, the serious wear has been principally confined to the threads of the screws engaged at readings below 0.5. Now, Mr. Christie has himself stated that from 1868, February 1, to 1869, December 31, there were only 283 observations of the Nadir made at and below readings of microscope A=0.4. If, therefore, we give the most liberal interpretation possible to the expression "the parts of the screw about 0.0," and understand it to mean all those parts of the screw which have been proved to be seriously defective, yet, even then, we should have less than 1,500 instead of 4,000 additional observations made near 0.0. But, in spite of these facts, Mr. Christie has thought himself justified in making such a statement as the following. "It seems a pity that Mr. Stone did not make himself better acquainted with the facts before making the confident assertion that 'whatever may have been the state of the screws near 0.0 in 1876 it certainly was not caused by the 4,000 observations made near 0.0 at my suggestion.'" It is extremely difficult to deal with such assertions in a way which can be regarded as satisfactory either to Mr. Christie or to myself.

In my opinion, neither 4,000 nor 6,000 additional Nadir observations made near 0.0 in a period of eight years would have led to any such serious wear of the screws as that indicated as existing in 1876 near 0.0. If all the observations were made

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